UNIVERSITY
OF MICHIGAN
SEP 5 1951
PERIODICAL
READING ROOM

THE JOURNAL OF PHILOSOPHY

VOL. XLVIII, NO. 17: AUGUST 16, 1951

CONTENTS

Philosophical Explication in Political Science: W. I. MATSON 513

Ethical Naturalism and Hedonics: RICHARD H. POPKIN 518

Book Review 524

RUDOLF CARNAP, *Logical Foundations of Probability:* ARTHUR W. BURKS.

Book Notes 536

Classic American Philosophers, Max Fisch, editor.

BENEDETTO CROCE, *Una pagina sconosciuta degli ultimi mesi della vita di Hegel.*

CONSALVO CECI, *Libertà ideale e libertà storica.*

PERCIVAL M. SYMONDS, *The Ego and the Self.*

New Books and Current Journals 539

PUBLISHED FORTNIGHTLY
BY THE
JOURNAL OF PHILOSOPHY, INC.

THE JOURNAL OF PHILOSOPHY

FOUNDED BY FREDERICK J. E. WOODBRIDGE AND WENDELL T. BUSH

EDITORS: Herbert W. Schneider, John H. Randall, Jr., Ernest Nagel.

BOOK EDITORS (1950–1953): Henry David Aiken, Virgil C. Aldrich, Joseph L. Blau, George Boas, C. West Churchman, Harry T. Costello, Robert D. Cumming, John R. Everett, Charles Frankel, Edwin Garlan, Albert Hofstadter, Paul O. Kristeller, Kenneth P. Landon, Harold A. Larrabee, Leroy E. Loemker, Donald S. Mackay, V. J. McGill, Ernest A. Moody, P. Romanell, Mario M. Rossi.

MANAGING EDITOR: Mrs. Iris Wilder Dean; EDITORIAL CONSULTANT: Emerson Buchanan.

THE JOURNAL OF PHILOSOPHY is owned by the Journal of Philosophy, Inc., and published fortnightly at Prince and Lemon Sts., Lancaster, Pa. Editorial Office: 515 West 116th St., New York 27, N. Y. President, John H. Randall, Jr.; Vice President, Ernest Nagel; Treasurer, Frederick A. Dewey; Secretary, Iris Wilder Dean.

Communications to the editors should be addressed to the Managing Editor, 515 West 116th Street, New York 27, N. Y. Subscriptions and advertisements should be sent to THE JOURNAL OF PHILOSOPHY, Prince and Lemon Streets, Lancaster, Pa., or 515 West 116th Street, New York 27, N. Y. The annual subscription price (twenty-six numbers) is $5.00. The price of single numbers is 30 cents, double numbers, 50 cents.

Entered as second class matter, March 18, 1921, at the Post Office at Lancaster, Pa., under Act of Congress of March 3, 1879.

PHILOSOPHY AND PHENOMENOLOGICAL RESEARCH

AN INTERNATIONAL QUARTERLY

OFFICIAL ORGAN OF THE
INTERNATIONAL PHENOMENOLOGICAL SOCIETY

Edited by
MARVIN FARBER
in cooperation with
A DISTINGUISHED GROUP OF AMERICAN AND FOREIGN SCHOLARS

This publication represents no special school or sect. Its aim is to maintain philosophy in the ancient sense, as an exact, descriptive discipline, at the same time bringing it to bear on the problems of the modern world. Space is provided for contributions representing philosophical tendencies in all countries. A series of translations of Russian articles on philosophy and psychology was inaugurated in December, 1944, and is being continued. The journal's significance for the various fields of scholarship makes it of interest to a wide reading public.

Published by the
UNIVERSITY OF BUFFALO
Buffalo 14, New York

Subscription price: Five Dollars per year

A descriptive circular listing the main contents of each number since the journal's foundation in September, 1940, may be obtained upon request.

VOLUME XLVIII, No. 17 AUGUST 16, 1951

THE JOURNAL OF PHILOSOPHY

PHILOSOPHICAL EXPLICATION IN POLITICAL SCIENCE

GOVERNMENT, law, and sovereignty are among the fundamental concepts that political science can not dispense with. Much writing about politics is not so clear as it might be because of failure to explicate the meanings of these terms, i.e., to indicate their relations to data of observation. It is the purpose of this paper to suggest explications of these notions.

Doubtless one reason why the attempt to analyze the observational content of these terms has seldom been made is that *prima facie* they do not seem to need it. It is almost always possible, for example, to specify exactly what persons constitute any given government, and also to state just what powers those persons have. A little reflection, however, discloses that government is not a quality inhering in the specified persons, but rather consists in very complicated relations of those persons to one another and to the governed. Historically, attempts to analyze these relations have taken the forms of power theory (e.g., Thrasymachus), contract theory (Locke), organism theory (Aristotle), or some combination of these (Hobbes, Marx). The well-known difficulties of each have caused tough-minded political scientists to ignore them, and to begin their treatises with "the facts"—descriptions of how actual governments function. But this will not do, because governments, laws, and sovereigns are not data. To begin in the middle of things is sound practice for the epic poet, but dubious in the scientist.

The concepts of law and government are clear enough to help us locate some, at least, of the data to which they refer. The difference that a law against X makes in my behavior is that I am discouraged from doing X; and the reason why I am discouraged is that I have been put on notice that if I do X the government will make things unpleasant for me. But things can be made unpleasant for me only by specific acts of specific persons. The question that interests us is the nature of the authority involved in this process, which is more than just the possession of mere superior force; but what is observed, i.e., the datum, is that somebody directs my activity into channels that it would otherwise not have taken. In the simplest case, only two persons are involved. Let

us call the relation *compulsion* and define it as follows: "*A* compels *B* to do *X*" means "*A's* will that *B* do *X* is a necessary causal antecedent of *A's* act *Y*, which occurs, and which in turn is a necessary causal antecedent of *B*'s act *X*, which also occurs."[1]

It can not be maintained that an act of compulsion, according to this definition, could be reported *entirely* in physical object sentences, because (*a*) "*A*'s will" is private to *A*, and (*b*) the notion of "necessary causal antecedent" is bound up with the complexities of contrary-to-fact conditionals. But behaviorists may, if they like, struggle with the first objection; and as the second difficulty is common to *all* the sciences, it seems justifiable to regard acts of compulsion as data.

It is convenient to define certain species of the genus compulsion. As a preliminary, we define *externally free act:* "*X* is an externally free act for *A*" means "It is impossible to predict *X* without reference to one or more laws of psychology in their particular application to *A*"; and "*X* is a deliberate act of *A*" means "*X* is externally free for *A*, and *A* is aware of this." We can now define *coercion* as compulsion in which the act *Y* (the compelling act) causes the act *X* (the act compelled) to be externally un-free for *B;* we shall use the word *inducement* to refer to compulsion that is not coercion. Inducement is subdivided into *threat,* in which the act *Y* consists of intimating to *B* that non-performance of *X* will cause *A* to *coerce B* to do some act *Z* (which will be unpleasant for *B*); and *persuasion,* which is inducement otherwise than by threat.[2]

The field of behavior that we have defined as compulsive is of course wider than that of the data of politics, as ordinarily understood. We shall find, however, that the field of political science can not be delineated on the data level, but must be discriminated with the aid of concepts that occur on the third level at lowest.

The low-level generalizations of compulsion must be laws concerning the efficacy of various modes of compulsion. Coercion is effective or not depending solely on the relative strengths and determinations of the parties involved, but threat is more subtle. Here the efficacy depends not only on brute force, but on the subjective estimate of it made by the person threatened, as well as his

[1] The definition, as given, applies to all kinds of friendly suggestions, and even teaching someone how to do something would be a case of "compulsion." The definition can be brought more into accord with usage, but the qualifications are too cumbersome to present in this short paper.

[2] Throughout these definitions, it is understood that "to do *X*" may be replaced by "to refrain from *X*" to obtain negative analogues.

estimate of the degree of unpleasantness of Z (the threatened consequence) in relation to X. Hence, the *laws* that are basic to political science will not be peculiar to that science. Coercion is mostly a matter of physics, but the laws of threat and persuasion fall mainly within the province of psychology. Certainly not all these laws are known, and those that are known are not capable of being stated with mathematical exactness. However, for the purpose of showing how we can advance from this level to the level of theory, the following observations will suffice:

1. Coercion is a very ineffective way of getting anything done. For human beings do things by moving their muscles, and these movements are in general externally free unless someone or something is directly constraining them.[3] In such circumstances, it is usually more practical for the coercer to do the act himself.

2. However, restraint (as we shall call the negative analogue of coercion) has a wider scope. Indeed, there is *no* act that can not be prevented by killing the would-be agent, which is the most extreme form of restraint. Imprisonment, posting of guards, erecting fences, and the like, also have their effectiveness. The principal disadvantage in their use as political methods is their nonselective nature; when A is restrained from doing X, it is usually the case that he is at the same time restrained from doing Y, which the restrainer may desire him to do.

3. One of the most convenient ways to insure behavior of a certain type is to prevent it from being deliberated. Hence the great importance of propaganda and education to governments and other organizations. However, the effectiveness of this approach is seriously reduced, almost nullified, unless a monopoly of propaganda and education can be maintained; and for this, methods other than persuasion are required.

4. On the whole, one man can not control another consistently and over a long period of time by coercion and threat alone. Hobbes was right in his insistence that the differences between men with respect to brain and brawn are not so great that political power can have its source in them alone.

We now turn to consideration of compulsive relations between more than two persons. Our fundamental concept is that of *organization:* a group of persons coöperating to achieve some end.

[3] It is important to note that by our definitions pointing a gun at a person is a case of threat, not coercion. It may be objected that this is an odd use of terms. I reply that the more usual distinction between "free" and "constrained" acts is incapable of being made with precision.

An organization is to be distinguished from a random group of persons all after the same thing by the existence of *discipline:* that is, the members of an organization recognize certain rules of behavior to be applicable to them as members, and an apparatus of compulsion is in existence to enforce them. Leaving persuasion out of the picture (because one who is thoroughly persuaded has no need of discipline) and also coercion (because of its inefficiency), we can classify organizations on the basis of the type and extent of *threat* involved in their discipline.

A *Type I* organization is one in which the threat can go no further than expulsion from the organization. Thus a member of a Type I organization can not be compelled to do X if X is more distasteful to him than exclusion from the organization. It is clear that Type I must be democratic, i.e., the sum of its aims and rules of discipline are consented to by all members. At worst a member may find himself in disagreement with these aims and rules, yet remain a member because the manner of making rules ("constitution") is such as to give him hope of modifying them in accord with his wishes.

A *Type II* organization is one in which discipline utilizes threats that go beyond expulsion from the organization, i.e., one with a forced membership; the members can not evade obeying the disciplinary rules of the organization by withdrawing from it.

It is evident that at a given place and time there can exist only one stable Type II organization. This is the State;[4] its disciplinary rules are the Law; those who formulate the law are collectively the sovereign; and the sovereign plus those who enforce the laws constitute the goverment. Law-enforcement, which is the typical governmental function, is possible only when those individuals constituting the government possess the requisite physical force to make good on the threats that are more politely denominated legal sanctions.

Space permits only one brief example of the sort of use to which this explication can be put. Let us discuss the question, What are the conditions of a stable tyranny?

We can distinguish within a Type II organization five subclasses, not all mutually exclusive: (1) the sovereign class, those

[4] Or sub-state, the authority in which is derived from one supreme organization. It is beyond the scope of this paper to discuss governmental hierarchies. A real *imperium in imperio* is factually, if not logically, impossible; even the medieval Church had to rely on the "secular arm" to compel membership.

who formulate the laws; (2) the soldiery, those who collectively possess (in their own persons) the preponderance of physical force; (3) the General Staff, those who issue orders to the soldiery; (4) the loyal, those who would still be members of the organization even if it were Type I; and (5) the disloyal, those who would *not* belong to the organization if it were Type I.

We observe: (*a*) the loyal and the disloyal are (at a given moment) mutually exclusive and exhaustive subclasses; (*b*) the necessary and sufficient condition of stability is that the classes General Staff, soldiery, and loyalists overlap to a degree that the disloyal neither control nor exercise a preponderance of physical force. Now a tyranny may be defined as a Type II organization in which the sovereign and General Staff classes are identical and have one member. The condition of stability then reduces to the requirement that the tyrant have effective control of the preponderance of force. Now how can this be? As a single man, he can by no means *exercise* the power himself. He must therefore be able to compel those who do. He can not do this by coercion, and his ability to do it by threat is limited. It is true that a man may be compelled by threat if he only *believes* that he may be coerced, when in fact coercion is impossible. But it is not possible for anyone to bamboozle a large group of men in this way for more than a short time. Hence the tyrant's control of the preponderance of power must be grounded in persuasion; that is, the exercisers of power must be made to believe that their interests are compatible with those of the tyrant. In so far as this is true, the group that exercises power must be held together by persuasion, which is almost to say that it must be democratic in effect. Even with modern armaments the group must be rather large.

This conclusion may not seem very startling; to be sure, it says hardly more than that a tyrant can not survive the disaffection of his army. However, it does serve to refute an important doctrine of Hobbes's theory of political power. For Hobbes advises men to set up an absolute sovereign to enforce the social contract. But even if men do covenant each with the other in some manner, explicitly or implicitly, they can not *literally* give up their power to the sovereign, nor can the sovereign literally enforce his will against the wishes of all his subjects, or even of very many of them, when the subjects in question are the General Staff and soldiery. Granted that covenants without the sword are but words, the sword that Hobbes demands has never been forged, nor will it ever be.

W. I. MATSON

UNIVERSITY OF WASHINGTON

ETHICAL NATURALISM AND HEDONICS

IN the current contest between the defenders of ethical naturalism and the defenders of ethical objectivism, there is often a fundamental confusion on both sides as to the nature of the ethical naturalist's position. The confusion is one of failing to distinguish between the naturalist's position as a scientific thesis and the naturalist's position as a philosophical thesis. The purpose of this paper is to show that the basic contention of ethical naturalism becomes clearer when such a distinction is made. Then it will be seen that the issue of whether or not certain scientific conclusions have adequate scientific evidence is irrelevant to the discussion of the validity of ethical naturalism.

The two naturalistic positions can be stated as follows: (*A*) The ethical naturalist's scientific thesis—it is possible to discover certain psychological, biological, and sociological laws which state regularities between the occurrences of value experiences and certain psychological, biological, and sociological factors. (This thesis could also be stated in some such form as, "A science of hedonics can be developed.") [1] (*B*) The ethical naturalist's philosophical thesis—the meaning of any ethical statement is the same as a statement reporting the occurrence of that set of "natural" [2] conditions which regularly accompany the value experience reported by the statement. (This thesis might be stated in the form "Ethics is hedonics.") [3]

My contention in this paper is that a confusion has grown up due to the fact that many naturalists have acted as if the evidence for (*A*) somehow constituted evidence for (*B*), and as if the objectivists' attack on this linking of (*A*) and (*B*), through their unveiling of the "naturalistic" fallacy involved, was an attack on the scientific theory (*A*). The result has been that the real signifi-

[1] I have borrowed this example from Arthur N. Prior's book, *Logic and the Basis of Ethics* (Oxford, 1949), p. 10. The claim might be that, rather than hedonics, a science of verbal emotional disturbances can be developed, etc. Any such science would consist of laws of the form, "Every time an individual human being reports the occurrence of a value experience in the form 'x is good,' or 'y is bad,' or 'z is pleasant,' or 'w is desirable,' or 't is beautiful,' a set of stimuli of type X also occurs."

[2] "Natural" refers to the type of term or factor dealt with in a science.

[3] Prior, at *loc. cit.*, suggests that the holder of the philosophical naturalistic thesis is offering hedonics as a substitute for ethics. It is perhaps more accurate to say that he is identifying hedonics and ethics, i.e., that he is claiming that the study of the former is the study of ethics. (This point seems to be the only place at which Prior's discussion of ethical naturalism would diverge from mine.)

cance of thesis (*B*) has been missed by both objectivists and naturalists.

To show this it is necessary to prove that the scientific thesis and the philosophical thesis are different. The evidence in support of the scientific thesis (*A*) is rather vast, consisting in the tremendous amount of anthropological, psychological and psychoanalytical, sociological, and biological discoveries of the last several decades. On the basis of this wealth of scientific detail, any one who still seriously doubts that scientists may, within a reasonable amount of time, develop a science such as hedonics, must base his doubts upon an examination of the scientific material, and not speculative arguments.[4] Ever since the crude formulations of laws regarding the occurrence of value statements and value experience by figures like Machiavelli, Hobbes, Hume, Bentham, Mill, Nietzsche, and others, it has been rather evident that there is some basis for believing that a naturalistic science of ethics of some sort is possible. The developments in recent years of the human sciences, and the applications of their results in modern advertising, thought control, etc., make it very difficult to doubt any longer that a science of hedonics, or something of the sort, is being developed.

It is evident that what I have appealed to in support of the plausibility of thesis (*A*) is a series of statements which are confirmed or denied by reference to the history, state, and application of findings in various scientific areas. In other words, this first thesis is defended or denied on nonphilosophical grounds.

The second thesis, the philosophical one, is of a different order entirely. The evidence that makes the scientific thesis highly probable in no way makes the philosophical one probable or improbable. The philosophical thesis does not depend upon whether scientists have or have not done certain things, or whether they will or will not do certain things, but only upon whether or not two different sets of terms, value and "natural" ones, do or do not describe the same object of experience. As G. E. Moore cogently explained in *Principia Ethica,* it just does not follow that since a set of factors regularly accompanies the occurrence of goodness, that set of factors is identical with goodness. To maintain that it does follow is to commit the "naturalistic" fallacy.[5] In other

[4] The old saw, frequently raised against thesis (*A*), that man is not a completely rational animal, is, of course, completely beside the point, since that would have nothing to do with whether or not laws could be found stating the condition of human value experiences.

[5] G. E. Moore, *Principia Ethica* (Cambridge 1948), p. 10.

words, the type of issue involved in the naturalistic philosophical thesis is not a scientific issue. Scientific results neither confirm nor deny that value experiences are identical with certain other "natural" experiences, or that the laws of hedonics are or will be laws of ethics. Scientific results only show that the "natural" experiences occur whenever value experiences do, and this is neutral to the issue of whether there is an identity between the two.

It is here that the basic confusion sets in. Many naturalists from the times of the Pyrrhonian sceptics, Thomas Hobbes, and others, have either implicitly or explicitly maintained that the evidence for the scientific theory established the philosophical one. The opponents of philosophical naturalism in ethics have roundly attacked the naturalists by showing that, in one way or another, they have committed the "naturalistic" fallacy. The attack has often been answered as if it were a denial of the scientific thesis, and therefore is properly rebutted by appealing to more scientific evidence. A debate along such lines bypasses the genuinely philosophical problem of analyzing and evaluating philosophical naturalism in ethics. The objectivist has alleged that he has refuted philosophical naturalism by showing that statements of the positions which argue for it by appealing to the merits of the thesis of scientific naturalism are fallacious. This refutes only a way in which the position has been defended, but not the position itself. The defenders, on the other hand, have not defended their castle, but somebody else's. To defend philosophical naturalism by defending scientific naturalism is not to defend the former at all. It is just not relevant to the question of whether philosophical naturalism is true to point out that it requires great ignorance and stamina to have serious doubts about the truth of scientific naturalism.

What then is evidence for or against the ethical naturalist's philosophical thesis? If I am correct in maintaining that this thesis asserts that ethics is hedonics or some other such science, or, in other words, that every ethical statement is equivalent in meaning to a statement containing natural terms, then this thesis is an epistemological rather than scientific one, and is to be evaluated accordingly. Since no past, present, or future scientific discoveries are relevant to whether or not ethical statements have the same meaning as certain natural ones, we must look for a different type of evidence. And I submit that this evidence is to be found or found wanting through an analysis of human experience which will reveal whether or not the identification of value experiences with certain natural ones is true to our actual experience.

Let us assume that the ethical naturalist's philosophical thesis

is something of the following sort: (1) all statements in which terms like "good," "bad," "beautiful," and "ugly" occur, are equivalent in meaning to statements containing no value terms, but only natural ones like "stimulus," "nervous tension," "conditioning," etc., and (2) all statements in which terms like "ought" occur are equivalent in meaning to disjunctive propositions containing only natural terms, of the form "Either —— happens, or such-and-such other events will occur." Such a thesis is not logically self-contradictory. It should be possible to construct an artificial language in which the terms are so defined that statements like "x is good" are equivalent to "A has nervous tension of degree n when tasting x," and others like "A ought to do y" are equivalent to "Either A will do y, or z will occur." The interesting question is whether or not such an identification corresponds to our experience. And here one can only appeal to his own intuitive experience to determine if this is the case.[6]

Certain objectivists like G. E. Moore think that it is just as intuitively obvious that "good" as experienced is different from natural qualities associated with it, as it is that a color as experienced is different from the light wave vibrations associated with it.[7] If they are right, this destroys the ethical naturalist's philosophical theory. However, as a believer in both the scientific and philosophical theory of ethical naturalism, I deny Moore's contention *solely* on the basis of my own intuitive experience. I find that I can not intuitively discover any distinction or difference in what I mean by statements like "x is good" and certain natural ones. Also I find that I know of no human value situation which can not be treated as if the identification between the value experience and the natural one were the case.

Thus, once the naturalist clearly distinguishes his scientific theory from his philosophical one, he can see what he must maintain to accept his position. Then he can decide for himself whether he is willing, on the basis of his experience, to maintain such a view. All scientific findings are irrelevant to the view, and it must be offered and maintained independently of them.[8] It may

[6] Some objectivists and naturalists might prefer a test by appeal to common-sense language to determine which view squares with our experience.

[7] *Ibid., loc. cit.*

[8] It is interesting to note that John Stuart Mill is one of the very few ethical naturalists who have done this. Cf. Everett W. Hall, "The 'Proof' of Utility in Bentham and Mill," *Ethics*, Vol. LX (1949–1950), pp. 1–18, and Richard H. Popkin, "A Note on the 'Proof of Utility' in J. S. Mill," *Ethics*, Vol. LXI (1950–1951), pp. 66–68.

be the case that once the two naturalistic theories are distinguished and separated, the psychological attractiveness of the philosophical theory may be lost. But this would only reinforce my contention that the two theories are different and are to be defended on totally different grounds.

If the naturalistic position is clarified, as I have suggested, then the objectivist can no longer rebut by pointing out the commission of the naturalistic fallacy, or argue, if he so wishes, against the scientific thesis.[9] Instead, the objectivist has to argue as Moore does, that our value experiences are of a different kind from natural experiences, or that there are human value situations which can not be treated honestly or adequately within a naturalistic theory.[10] The former is difficult or impossible to show to some one who does not already believe it, since if it is not part of his intuitive experience, it is hard to see how it can be made so. All that can be done by the objectivist is to point out times when he finds the distinction between value experiences and natural experiences is obvious, and trust or hope that his opponent will do likewise. The second type of argument—that there are value situations that can not be adequately dealt with by a naturalist—is a more forceful point. It can be presented strikingly in situations where disagreements about moral matters are occurring, or in situations where moral advice is being given. Since the "disagreement" when translated according to some versions of the naturalistic theory is no longer a disagreement, it is difficult for such a naturalist to explain what is being disputed. Thus, for example, if X asserts "a is good," and Y asserts "a is bad," and these statements are treated as equivalent in meaning to statements of the form "I, X, have strong feelings of approval in regard to a" and "I, Y, have strong feelings of disapproval in regard to a," then, X and Y are not disagreeing, but reporting different feelings. A similar sort of problem arises in the case of moral advice, where a statement of the sort "A ought to do x" is often interpreted by the naturalist as equivalent in meaning to either a statement about the advisor's state, or a prediction by the advisor as to what will happen if A does not do x. The obligatory feature of the advice has disappeared in either rendition. Pointing out these sorts of situations

[9] Many objectivists have no doubts about the scientific thesis. See, for instance Moore, *loc. cit.* This indicates again the logical independence of the two naturalistic theses since an objectivist can perfectly well accept the scientific one while denying the philosophical one.

[10] For example, see Brand Blanshard's recent article "Subjectivism in Ethics—A Criticism," *Philosophical Quarterly*, Vol. I (1950–1951), pp. 127–139.

does not refute ethical naturalism, but only indicates some of the odd consequences of the position. The naturalist, in answer, may, as I would, acknowledge the odd consequences and accept them, or reply as Bertrand Russell did, "while my own opinions as to ethics do not satisfy me, other peoples' satisfy still less."[11]

The objectivist must do more than point out that the naturalistic position has often been stated badly, or that it has some peculiar consequences. (For what theory does not?) He must also make a positive effort to establish an alternative thesis—that one or more value terms are irreducible and that only a theory that admits this actually squares with human experience.

Since it is not the purpose of this paper to argue for or against ethical naturalism, but only to attempt to clarify what the position is, I shall now restate my contention. Ethical naturalism is really two theories, one a scientific thesis that a science such as hedonics can be developed, and the other a philosophical thesis that all value statements are equivalent in meaning to statements in hedonics. Both the proponents and opponents of ethical naturalism have treated these theses as if they were the same, or as if evidence for the first supplied evidence for the second. The opponents have, on this basis, been able to detect the commission of the naturalistic fallacy in the arguments of the naturalists. The proponents have acted as if the opposition was to their scientific thesis, and possibly, in addition, was intended to prove the existence of absolute values, and, hence, have answered by appealing to the vast amount of scientific evidence in support of the scientific thesis. Since, however, ethical naturalism as a philosophical thesis is an epistemological rather than a scientific contention, the discussion on the above level is irrelevant. The philosophical thesis must be accepted or rejected on its epistemological merits alone. Here one must be his own judge by deciding if, as a claim about the character of our knowledge, the philosophical thesis is true to the actual experience that one has. Both the objectivist and the naturalist could accept the same science of hedonics as a statement of laws that obtain between the occurrence of value experiences and natural experiences. However, they disagree as to whether or not the value experiences are the same as the natural experiences, and it is here that scientific findings are irrelevant as support for either side.

RICHARD H. POPKIN

STATE UNIVERSITY OF IOWA

[11] Bertrand Russell, "Reply to Criticisms," in *The Philosophy of Bertrand Russell*, edited by Paul Arthur Schilpp, Library of Living Philosophers, Vol. V (Evanston and Chicago, 1944), p. 724.

BOOK REVIEW

Logical Foundations of Probability. RUDOLF CARNAP. Chicago, Illinois: The University of Chicago Press. 1950. xvii + 607 pp. $8.50.

I

In this book Professor Carnap begins the development of a logical system of probability and a philosophical theory of induction which are of great importance for philosophers, mathematicians, and scientists alike. This development is to be completed in a successor volume (on systems of inductive logic), but the results presented here are for the most part complete in themselves and serve to give a clear picture of Carnap's novel and interesting approach to probability.

The wealth of valuable material in this book is too great to summarize adequately here; we shall present only the salient features of Carnap's results, laying stress on those of most interest to readers of this JOURNAL. Carnap begins by distinguishing what he believes to be the two main meanings of the word 'probability': *probability*$_1$ is degree of confirmation, and *probability*$_2$ is relative frequency in the long run. His principal aim in this book is to find an analysis (in his terminology, an adequate *explicatum*) for probability$_1$. The idea is that the *explicandum* probability$_1$ is a vague, unformalized concept and that it should be replaced by a precise, formalized explicatum, a concept of degree of confirmation. Carnap's procedure is to construct, step-by-step, a semantic definition of a particular degree of confirmation function c^* and then to propose c^* as a quantitative explicatum for probability$_1$.

We must raise, at the outset, an objection to Carnap's use of the concept of probability$_1$. His definition of 'probability$_1$', which may best be presented in two parts, is: (1) "Probability$_1$ is the degree of confirmation of a hypothesis h with respect to an evidence statement e, e.g., an observational report", and is to be contrasted with probability$_2$, which "is the relative frequency (in the long run) of one property of events or things with respect to another" (p. 19); (2) A simple sentence about probability$_1$ is analytically true or false while a sentence about probability$_2$ is synthetic (factual). Now Carnap claims that the word 'probability' is often used in inductive arguments in the sense of 'probability$_1$', but he gives no grounds for this claim, presumably because it seems to him sufficiently evident to need no justification. It is by no means evident, however, that the word 'probability' is ever used in inductive arguments in Carnap's sense of 'probability$_1$', and it must be recognized that much of philosophical importance is by-passed by any theory

of induction which takes probability$_1$ [as defined by *both* (1) and (2)] as its starting point.

The confusion arises out of not sufficiently distinguishing (1) and (2). Let 'probability$_{I(nduction)}$' be defined by (1) alone, i.e., by rewriting (1) with 'probability$_I$' replacing 'probability$_1$'; it is an essential feature of the definition of 'probability$_I$' that it leaves open the question as to whether a sentence about probability$_I$ is analytic or synthetic. 'Probability$_I$' thus gives the sense of 'probable' as this word is used in a primary way in the conclusions of inductive arguments ("Therefore, it is probable that . . .") and in assertions that a given theory is probable to a certain degree relative to known evidence (e.g., "The theory of relativity is highly probable relative to the evidence now available"). Therefore there can be no doubt that the word 'probability' is often used in the sense of 'probability$_I$'.

However, because condition (2) is included in the definition of 'probability$_1$' the claim that the word 'probability' is often used in the sense of 'probability$_1$', far from being obvious, is exactly the point at issue between the frequency and *a priori* theories of probability$_I$. For anyone who gave a frequency analysis of the primary use of 'probable' in the conclusions of inductive arguments would of course deny that the word 'probability' is ever used in the sense of 'probability$_1$', i.e., that it is ever used in actual inductive arguments in such a way that sentences about it are analytically true or false. (The frequentist would not deny that the a priorist thinks he uses the terms in that way—he would argue that the a priorist is mistaken in that belief.) Peirce, for example, applied the concept of relative frequencies to inductive arguments and thus was analyzing neither probability$_1$ (since probability$_1$ is not a frequency concept of probability) nor probability$_2$ (since probability$_2$ as defined by Carnap is applicable to properties of events or things, not to arguments), though he was analyzing probability$_I$. Moreover, Peirce rejected as irrelevant to induction the a priorist's procedure of counting logically possible universes, on the ground that universes are not as plentiful as blackberries.

Hence drawing the distinction between probability$_1$ and probability$_2$ does not settle the issue between the frequentist and the a priorist concerning the nature of probability$_I$, as Carnap seems to think it does. It is important to recognize with Carnap that much of what the frequentist says about probability is not about inductive arguments (and so not about probability$_I$) at all, but about relative frequencies of properties of events or things (i.e., about probability$_2$), but it is a mistake to think that this is true of all he says. Similar remarks concerning the concept of probability$_1$ hold

for pragmatic theories of probability (Ramsey, de Finetti), for though they take the concept of expected value as basic rather than the concept of probability, the concept of probability that they derive from that of expected value is an analysis of neither probability$_1$ nor probability$_2$ but of probability$_I$.

It is very important in discussing a work containing both technical logical developments and philosophical discussions based on these to distinguish the two. This we shall do by presenting, in Section II, (*A*) Carnap's logical system of probability; and in Section III, (*B*) his philosophical theory of induction, i.e., his theory of probability$_I$. The two are closely connected, of course, for (*B*) is based on a proposed interpretation of the formal semantical system (*A*), but the difference is as fundamental as (indeed, it is a special case of) the difference between mathematics (including mathematical logic) and philosophy. Though Carnap develops his logical system of probability not postulationally but rather by means of the semantic techniques which he and others have created, the difference between (*A*) and (*B*) is the difference that normally appears in works on probability between the calculus of probability and the theory about how this calculus should properly be interpreted.

Of course, the kind of calculus one wants depends on the intended interpretation. Nevertheless, the criteria for judging a calculus are very different from those for judging a theory of probability$_I$; the former are mathematical and logical in character (consistency, validity of proofs, etc.) while the latter are philosophical (the adequacy of the theory to explain inductive arguments, etc.). Thus the frequentist can accept the a priorist's calculus as a valid mathematical system without in the least being committed to his philosophical theory of probability$_I$, and vice versa. Similarly, everyone will accept Carnap's logical system of probability as a very significant addition to formal semantics, whether or not he accepts Carnap's version of the *a priori* theory of probability$_I$.

II

Carnap begins the construction of his logical system of probability by confining himself to languages in which individual variables are the only variables, and in which there are a finite number of primitive predicates, that is, to forms of the lower functional calculus including relations and identity (p. 58). There are two cases to be distinguished as requiring somewhat different treatment: a language L_N contains a finite number of individual constants ('a_1', ..., 'a_N'), while a language L_∞ contains a denumerable infinity of individual constants ('a_1', 'a_2', ...). (Carnap uses

German letters for technical symbols in the metalanguage, but for typographical reasons we do not do this.) Degree of confirmation functions are defined first for the finite case and then extended to the infinite case by a limiting process.

The formal development proceeds in the following way. An *atomic sentence* is defined as any sentence made by juxtaposing a predicate (of any degree) and the proper number of individual variables and/or constants (e.g., 'Ha_1'). A *basic pair* consists of an atomic sentence and its negation (e.g., 'Ha_1' and '$\sim Ha_1$'). A *state-description* (Z) in L_N "is a conjunction which contains as components exactly one sentence from every basic pair in L_N and no other sentences, these components being arranged in their lexicographical order" (p. 72). For example, there are eight distinct state-descriptions in a system L_3^1 with a single monadic predicate 'H' and three individual constants. See the accompanying table, where 'T' abbreviates '$\sim$ H' and where for convenience we employ a non-lexicographical order in writing state-descriptions.

State-description (Z)	$c_0^*(Z)$	$c_0^\dagger(Z)$
$Ha_1.Ha_2.Ha_3$	1/4	1/8
$Ha_1.Ha_2.Ta_3$	1/12	1/8
$Ha_1.Ta_2.Ha_3$	1/12	1/8
$Ta_1.Ha_2.Ha_3$	1/12	1/8
$Ha_1.Ta_2.Ta_3$	1/12	1/8
$Ta_1.Ha_2.Ta_3$	1/12	1/8
$Ta_1.Ta_2.Ha_3$	1/12	1/8
$Ta_1.Ta_2.Ta_3$	1/4	1/8

A regular *c*-function for a language L_N is any function constructed in the following manner. To each state-description a positive real number is assigned so that the sum of these numbers over the state-descriptions is 1. In Carnap's terminology the assigned real number is the probability of the state-description relative to the tautological evidence, or the null confirmation ['$c_0(Z)$'] of that state-description. Two possible assignments of c_0-values for language L_3^1 are shown in the table. (We skip the step whereby Carnap uses a neutral symbol '*m*' and shows later that c_0 coincides with *m*; p. 294). The null confirmation of any sentence is then defined as the sum of the c_0-values for those state-descriptions in which the sentence holds. For example, c_0^*('$Ha_1.Ha_2$') = 1/3 in L_3^1 since '$Ha_1.Ha_2$' is logically implied by the first and second state-descriptions in the table. Finally, the degree of confirmation of an hypothesis h on evidence e in language L_N is defined by

$$c(h, e) =_{Df} \frac{c_0(e.h)}{c_0(e)}.$$

For example,

$$c^*(\text{'Ha}_3\text{'}, \text{'Ha}_1.\text{Ha}_2\text{'}) = \frac{c_0^*(\text{'Ha}_1.\text{Ha}_2.\text{Ha}_3\text{'})}{c_0^*(\text{'Ha}_1.\text{Ha}_2\text{'})} = \frac{3}{4}.$$

[If $c_0(e) = 0$, that is, if e is logically false, then $c(h, e)$ is not defined.]

This shows how a c-function may be defined for the finite case. The definition for the infinite case is accomplished in the following ingenious way. Consider again L_∞, which contains the denumerable infinity of individual constants 'a_1', 'a_2', ... Then form the sequence of languages L_1, L_2, ..., of which L_N has the same predicates as L_∞ but only the individual constants 'a_1', ..., 'a_N'. Thus each language of the sequence is a part of each of its successors and of L_∞. Now, for every L_N a c-function can be defined, so that we can generate a corresponding sequence of c-functions for any given pair of sentences h and e: ${}_1c(h, e)$, ${}_2c(h, e)$, ... Then $c(h, e)$ in L_∞ is defined as the limit of this sequence, if a limit exists.

The next step of the formal development is to restrict the initial assignment of c_0-values to state-descriptions in such a way that *isomorphic* state-descriptions receive the same values. 'Isomorphic' is, of course, given a careful technical definition, but the essential point is that we can pass from one isomorphic state-description to another by a systematic replacement of individual constants and a lexicographical rearrangement. For example, by substituting 'a_2' for 'a_3' and 'a_3' for 'a_2' in '$\text{Ha}_1.\text{Ha}_2.\text{Ta}_3$' and rearranging we get '$\text{Ha}_1.\text{Ta}_2.\text{Ha}_3$', which is a state-description isomorphic to the one with which we began. In the table all state-descriptions isomorphic to one another are grouped within the same bracket. Any regular c-function constructed so that isomorphic state-descriptions receive the same c_0-values is called a symmetrical c-function; in particular, c^* and $c\dagger$ are symmetrical c-functions (see the table). The definition is extended to L_∞ in the same way as before.

We are now in a position to define c^*, that c-function which Professor Carnap offers as an explicatum for probability$_1$. Again, the technical definition is complex, but the general idea is beautifully simple. The disjunction of all the state-descriptions isomorphic to a given one (in lexicographical order) is called a *structure-description*. Let τ be the number of structure-descriptions in the system; e.g., in the table the number (4) of bracketed groups of isomorphic state-descriptions. Let ζ_i be the number of those state-descriptions which are isomorphic to a given state-description Z_i. Then

$$c_0^*(Z_i) =_{Df} \frac{1}{\tau\zeta_i},$$

and c^* in L_N is defined on the basis of c_0^* as before. Thus c^* is that symmetrical c-function for which the c_0-value of every structure-description is the same. The definition is extended to L_∞ in the same way as before; in his successor volume Carnap will show that the limit exists for c^* where non-general sentences are the arguments and for at least a large class of general sentences.

It should be apparent from this outline of Carnap's logical system of probability that it is a most important and original contribution to inductive logic.

III

As we remarked earlier there is nothing controversial about the preceding logical system regarded as a formal semantical system. The explicit definition of c^* is rigorously stated, and given any h, e, and L_N one can in principle calculate the value of $c^*(h, e)$ in L_N. There can be no general procedure for calculating c^*-values in L_∞,[1] but any sentence of the form '$c^*(h, e) = x$ in L' is analytically true or false (whether L is L_∞ or L_N). We do not have a philosophical theory of induction until we come to Carnap's proposed interpretation of the system, in particular to his claims that c^* is an adequate explicatum for probability$_1$ [or probability$_I$] and that the word 'probable' is often used in inductive arguments in the sense of probability$_1$ (cf. supra).

From the semantical method employed in the construction of Carnap's logical system of probability we know that the symbols of the language systems for which c^* is defined are to be interpreted in the normal manner: individual constants are to designate individuals, predicates are to designate properties, and the logical symbols are to be assigned their usual meanings. Such semantical rules by no means suffice to take us from the formal definition of c^*

[1] Carnap points out that this follows from Church's result and that in this respect the general situation in inductive logic is no different from that in deductive logic, for likewise there can be no general mechanical procedure for deciding of sentences of L_∞ whether or not they are theorems. Carnap comments that there is thus a fundamental logical restriction on the type of machine that can be constructed to do inductive and deductive logic, though he adds that in both fields there can be methods of decision restricted to special classes of sentences and machines to employ those methods.

It is worth noting that there is an alternative way of using machines to solve logical problems, that of employing non-effective methods similar to the methods human beings employ. Thus the non-existence of a general decision procedure does not preclude the construction of a logical machine which will constantly grind out interesting questions and answers to them, or which, given any question, can in a useful fraction of cases discover the answer to it. Human beings have these powers, so it is not *a priori* precluded that machines could be built to have them. Whether this is in fact possible or not is, of course, a different matter.

to what is ordinarily meant by 'probability$_1$', however, as Carnap recognizes and as the following consideration shows. In judging the probability status of a specific hypothesis in a specific context we assign only one value of probability$_1$ to it; to assign more would be to make rational behavior impossible. But the rules of Carnap's logical system of probability do not determine a unique c^*-value (degree of confirmation) for an hypothesis but an infinity of such values: the fundamental concept that has been defined is 'c^*(h, e) = x in L' so that the c^*-value of an hypothesis depends on *both* the evidence e and the language L (pp. 283–284). Carnap provides three further rules of interpretation to take care of this situation; he calls them "requirements" and says that they belong neither to deductive logic nor to inductive logic but to the methodology of logic (pp. 73, 211).

The first rule is *the requirement of logical independence* (p. 72): The individual constants of L must be interpreted so that they designate different individuals, and the primitive predicates must be interpreted so that the properties (monadic and relational) they designate are (*a*) logically independent of one another and (*b*) purely qualitative (non-positional or non-indexical). The purpose of the requirement of logical independence is to insure that the symbols of the system will be so interpreted that the state-descriptions shall describe logically possible states of the universe of L. It will be interesting to interpret, in the light of this fact, the concepts of a regular *c*-function, a symmetrical *c*-function, and the particular function c^*.

Assigning numbers to the state-descriptions in constructing a regular *c*-function amounts to assigning a non-zero *a priori* or initial probability to each possible state of the universe. More generally, 'c_0(h) = x in L' tells us the probability of a sentence in L being true before any factual evidence is available (p. 308). There is thus in Carnap's system a sense of probability which is *not* relative to *empirical* evidence [Carnap speaks of "tautological evidence" (p. 289) but this is a deviation from the ordinary use of "evidence"] and in this respect Carnap's version of the *a priori* theory of probability$_1$ differs from all other versions of that theory (so far as the reviewer is aware). Yet in proposing non-relative probabilities Carnap shows a better insight into the implications of the *a priori* theory of probability than earlier advocates of that theory. For the existence of non-relative probabilities seems implied by the traditional principle of indifference (also called the principle of non-sufficient reason and—more appropriately—the principle of the equal distribution of ignorance), since according to this principle equal probabilities are to be assigned to alter-

natives if there is no positive reason to assign unequal probabilities, and surely when there is no evidence at all there is no positive evidence that unequal probabilities should be assigned. However, the traditional principle of indifference was never stated formally and hence none of the axiom systems for probability$_1$ (e.g., those of Keynes and Jeffreys) assigned degrees of null confirmation (initial probabilities) to sentences. It follows that these axiom systems hold for all regular c-functions. These form a very broad class, as can be seen by the fact that for every real number x between 0 and 1 there is a regular c-function such that $c(\text{'Ha}_3\text{'}, \text{'Ha}_1.\text{Ha}_2\text{'}) = x$ in L_3^1.

The passage from regular to symmetrical c-functions was achieved by assigning equal c_0-values to isomorphic state-descriptions. As Carnap notes, this amounts to treating all individuals the same (pp. 485, 488). He argues that such a principle is never questioned in deductive logic and seems equally reasonable in inductive logic. However, there is more involved in this step than that. The individuals designated in the languages chiefly discussed in this book do not constitute an ordered system, but if they did we could legitimately speak of a temporal order of events. (Carnap has a few remarks on such systems and promises to make such an extension in Volume II.) In that case treating all individuals the same would involve one aspect of the traditional uniformity of nature principle: whatever causal or inductive connections there may be are invariant with respect to time. (Cf. in this regard the restriction to non-positional properties involved in the requirement of logical independence.)

Finally, c^* is that particular symmetrical c-function for which the c_0-value of every structure-description is the same. Now each structure-description describes, roughly speaking, a possible structure of the universe under consideration. Thus c^* assigns to each possible structure the same initial probability. In this respect it is to be contrasted with the function $c^\dagger$ which assigns the same c_0-values to all state-descriptions. This latter is the assignment that results from attaching the probability of one-half to any atomic sentence, independently of what other atomic sentences are true; for example, in L_3^1

$$c_0^\dagger(\text{'Ha}_3\text{'}) = c^\dagger(\text{'Ha}_3\text{'}, \text{'Ha}_1.\text{Ha}_2\text{'}) = c^\dagger(\text{'Ha}_3\text{'}, \text{'Ta}_1.\text{Ta}_2\text{'}) = \frac{1}{2}.$$

Thus, as Carnap remarks, "the choice of $c^\dagger$ as the degree of confirmation would be tantamount to the principle never to let our past experiences influence our expectations for the future" (p. 565). It is clear that we do regard what happens in the past as indicative

of what will happen in the future; this also involves the traditional uniformity of nature postulate.[2]

The next rule of interpretation is *the requirement of completeness* (pp. 74–75): No language L is to be used for purposes of inductive logic in connection with any universe U unless L is sufficiently rich to express all qualitative attributes exhibited by the individuals of U. An important implication of this requirement needs to be noted. The languages to which Carnap's logical system of probability applies contain only a finite number of primitive predicates. Hence either the system must be extended to include languages with an infinity of primitive predicates, or it must be "assumed that any two individuals differ only in a finite number of respects" (p. 74). The latter assumption is expressed by Keynes's principle of limited variety, and Carnap argues that it is a reasonable assumption to make (p. 75).

One may wonder why the requirement of completeness is not expressed simply by stating that no language is to be used for purposes of inductive reasoning unless it is just adequate (neither too weak nor stronger than necessary) to permit a complete description of the actual universe. Carnap does not discuss this formulation, but it seems clear that ideally the principle should be formulated in this way. However, such a formulation would be inappropriate at the present stage of development of Carnap's logical system of probability; for c^* is defined only for languages which lack variables for real numbers, and these are inadequate for expressing the theories of quantitative science. Professor Carnap is aware of this limitation and hopes that it will sometime be removed. He admits that such an extension involves serious difficulties, but sees no reason to regard them as insuperable (p. 519). He points out, correctly, that any such program must begin with the simpler languages and that a further development of deductive logic is needed as a basis for subsequent improvements in inductive logic.[3]

There is an old criticism of the *a priori* theory of probability, however, which ought not be overlooked at this point. Any *a priori*

[2] Here the principle is stronger than in the case of treating all individuals the same: it states not only a temporal invariance (both c^* and $c\dagger$ share this property), but also that what happens in the past constitutes evidence for what will happen in the future (c^* has this property but $c\dagger$ does not).

[3] For example, "the deductive logic of relations, although widely developed in other respects, is today unable to give us a general formula stating the number of structures of one dyadic relation for finite N, let alone the same for several relations" (p. 124). As a consequence, though the definition of c and some theorems developed in this book apply to any language of the kind stipulated, most of the theorems applicable to inductive inferences apply only to languages with monadic properties.

theory must employ the principle of indifference (whether it is called by that name or not) in the following sense: an *a priori* theory is committed to assigning probabilities to sentences relative to no evidence or to so-called symmetrical evidence. Now the problem is to make such an assignment in a manner that does not lead to contradictions. Keynes indicated how this could be done for the qualitative (discrete) case, and Carnap has demonstrated constructively how to do it for this case. But no one has yet given the least indication of how it can be done for the quantitative (continuous) case,[4] so that there is a fundamental difficulty with the *a priori* theory of probability$_I$.

Carnap's philosophical theory of induction is inadequate in another respect. Suppose we were in the possession of a language which is neither too weak nor stronger than necessary completely to describe the actual universe.[5] Then science would have the basic picture of the universe and would be reduced to filling in details. Thus a tremendous amount of scientific knowledge is presupposed by the requirement of completeness, to which (by the very nature of the requirement) c^* is not properly applicable. Carnap says that it is an epistemological question as to whether or not the requirement of completeness is fulfilled (p. 75). But this is taking far too much out of science and putting it into epistemology. The problem of finding a complete and adequate set of properties for describing natural phenomena is as much a part of science as the problem of finding what connections obtain among these properties. Of course, the solution of either of these problems involves epistemological and methodological considerations; but the statement 'It is highly probable on the basis of present evidence that a certain language does permit a fairly complete description of

[4] An example of how the principle of indifference leads to contradictions where continuous quantities are involved is the following. Suppose that all that is known of a certain substance x is the statement: (E) the specific density of x lies between 1 and 2. Consider now the proposition: (A) the specific density of x lies in the interval 1 to 3/2. By the principle of indifference the probability of the specific density of x being in the interval 1 to 3/2 is equal to the probability of its being in the interval 3/2 to 2; hence probability (A, E) = 1/2. But if the specific density of x lies between 1 and 2 then the reciprocal of the specific density of x is in the interval 1 to 1/2. Consider next the proposition: (B) the reciprocal of the specific density of x lies in the interval 1 to 2/3. Applying the principle of indifference in this case (since the intervals 1 to 5/6, 5/6 to 2/3, 2/3 to 1/2 are all equal) we get probability (B, E) = 2/3. But (B) is logically equivalent to (A), so probability (A, E) = 2/3, which contradicts our previous result. For a discussion of this and similar examples see J. M. Keynes, *A Treatise on Probability*, pp. 45 ff.

[5] There will, of course, be several such languages, and we do not know whether c^*(h, e) would have the same value in all of them.

the universe' clearly belongs to inductive logic, and a satisfactory logical system of probability must accommodate it.

Another way of putting the point is this. One reason, and an important one, for the uncertainty (in the sense of probability$_1$) with which a scientific generalization is held is that not all properties exemplified in the universe are known and some that are causally relevant may have been left out of consideration; if they were known, their effect could be tested. Correspondingly, the truly revolutionary developments in science (e.g., those made in mechanics by Galileo, Newton, and Einstein) involve the invention of basically new concepts, that is, the construction of fundamentally new languages; and part of the grounds for the uncertainty of a scientific theory are to be found in the fact that further improvements in this regard are most certainly to be made. Thus it is a consequence of the requirement of completeness that until science gives us a language completely adequate to describe the universe Carnap has not really given us an analysis of probability$_1$. For consider the sentence, expressed in a scientific language L_s: (*a*) 'The probability of hypothesis h relative to all the available evidence e is x'. The essential claim of Carnap's philosophical theory of induction is that c^* is an adequate explicatum for probability$_1$, and this amounts to saying that the proper analysis of (*a*), expressed in a metalanguage, is (*b*) 'c^*(h, e) = x in L'. By the requirement of completeness the language referred to in (*b*) is not L_s but some not yet existent perfect language! What seems to be needed here is a theory of induction capable of comparing various languages with respect to their expressive powers, simplicity, etc.

The third rule for the application of Carnap's logical system of probability to inductive arguments is *the requirement of total evidence:* "in the application of inductive logic to a given knowledge situation, the total evidence available must be taken as basis for determining the degree of confirmation" (p. 211). This is a precise formulation of the traditional dictum that in induction we should take into account all the information we have, and it is an acceptable way of handling the relativity of the probability$_1$ of an hypothesis to the evidence. It is a minor objection that the principle is *ad hoc:* it would be desirable to justify it by showing that in some sense using all the available evidence maximizes our chance of success or the reliability of our inductive judgments, i.e., in some sense gives us the best prediction possible under the circumstances.

IV

It should be emphasized that the preceding discussion does not do justice to the large amount of important and interesting material

in Carnap's book. We have only skimmed over the chapters on the quantitative concept of probability, leaving out the many important detailed results of scientific and philosophic interest to be found there (the proofs of theorems—e.g., Bernoulli's—, the discussions of analogy, induction by simple enumeration, and others of the traditional inductive argument forms, etc.). There are also chapters on the comparative and classificatory concepts of probability, on the relevance and irrelevance of information (i.e., the effect of adding new information to e on the degree of confirmation of h), and on estimation and the reliability of estimates of magnitudes of various kinds, including frequencies. And all of this is preceded by a long and useful chapter on deductive logic which lays the foundations for the subsequent formal developments.

There are also in the book many interesting discussions of Carnap's and others' philosophical theories of induction. These parts of the book have by no means the completeness and adequacy of the parts devoted to the logical system of probability, however. We have already noted several topics that stand in need of fuller consideration, and we shall mention one more in closing. There is a section devoted to the claim that no synthetic presuppositions (e.g., the principle of the uniformity of nature) are needed to justify the validity of inductive inference. This is a crucial point, for if synthetic presuppositions are needed to justify induction, simple probability$_1$ sentences are not analytic, c^* is not an adequate explicatum for probability$_1$ (since simple c^* sentences are analytic), and hence Carnap's theory of induction is incorrect. Yet Carnap does not show how one can justify, in a non-circular manner, and without the use of synthetic presuppositions, the claim that one c-function is more useful than another. For the most part Carnap leaves the reasons for the choice of c^* to Volume II, so that a complete discussion was not to be expected here. But many choices in the construction of his language system which involve presuppositions are passed by without any recognition of this fact: the assumption of limited variety, the restriction of predicates to those designating non-positional or non-indexical properties, and the treatment of all individuals as equals in the restriction to symmetrical c-functions. It is to be hoped that Professor Carnap will discuss all of these questions more adequately in Volume II.

Whether or not Carnap's theory of induction is the correct one, there can be no doubt that this book is a landmark in the history of its subject.

ARTHUR W. BURKS

UNIVERSITY OF CHICAGO

BOOK NOTES

Classic American Philosophers. Peirce, James, Royce, Santayana, Dewey, Whitehead. Max H. Fisch, general editor. Selections from their writings with introductory essays by MAX H. FISCH, ARTHUR W. BURKS, PAUL HENLE, GAIL KENNEDY, OTTO F. KRAUSHAAR, VICTOR LOWE, PHILIP B. RICE. New York: Appleton-Century-Crofts, Inc. 1951. x + 493 pp. $4.75.

The "classic" philosophers included in this new volume in the Century Philosophy Series are Peirce, James, Royce, Santayana, Dewey, and Whitehead. This reviewer has some doubts about the propriety of including Whitehead as an American philosopher. As Victor Lowe points out, "His life in America did not begin until 1924, when he was sixty-three years old" (p. 395) and approaching retirement. The question of propriety is raised here merely for the record; the practice of teachers of philosophy supports the decision of the editors of this useful text in their decision to keep Whitehead as one of the canonical six. With this one reservation, it seems to me that there can be no doubt of the superior claims of these philosophers to be considered representatives of a classical period in American philosophy.

Fisch has written a general introduction, defining the period from, roughly, 1870 to 1940 in terms of the personal and intellectual relations of the six, the cultural and social environment in which their work was done, and the philosophic trends which they initiated or to which they gave classic formulation. Fisch's introduction is a careful and sane statement which can be read with profit by those who are beyond their student years. There should, perhaps, be more said of the realistic interlude in the early years of the twentieth century which has left as its residue a realistic temper in later American thought. Certainly the epistemological inquiries of the new realists and critical realists had broader consequences in America than simply the fact that they were coöperatively prosecuted inquiries.

After this general introduction, each of the six is discussed in a detailed introductory essay by one of the editors, and represented by between 50 and 60 pages of extended selections. Each editor has had a free hand in organizing his introduction and making his selections. There is, as a result, considerable variety in the approaches to each of the six philosophers. Were this book intended as a series of definitive studies, this variation would be a serious difficulty. Since, however, the text is meant as an introduction to recent American philosophy, the stylistic differences and differences

in mode of presentation and criteria of selection simply add to the readability of the book as a whole.

Finally, there is a series of useful brief bibliographies, listing the chief works by and about each of the philosophers included, and an index which is complete enough for the purposes of the beginning student. On the whole, it is a book which merits and should have wide use in the many courses in American philosophy now being given at colleges over the country.

J. L. B.

Una pagina sconosciuta degli ultimi mesi della vita di Hegel. Con un'appendice. BENEDETTO CROCE. Bari: Laterza. 1950. 67 pp.

The three pieces contained in this slim but substantial fascicle ("An unknown page from the last months of Hegel's life"; "On the so-called philosophical reforms, and in particular on that of Hegel"; "The present-day existentialist Hegel-Renaissance") will be invaluable to any teacher, critic, or general reader who desires to have at hand, in the tersest possible form, a résumé of what Croce deems usable or rejectible in Hegel's system. To Croce connoisseurs, this *cahier* will offer little novelty. There is the usual opposition, on the one hand, to pure conceptualism, on the other, to naked factualism; the affirmation of the unity of history and philosophy; the customary exorcization (surprising in a thinker like Croce, who has devoted years of strenuous efforts to expounding and interpreting the works of Vico, the "founder" of historical philosophy) of the philosophy of history; the advocacy of philosophy as a logic of history, i.e., as clarification of the concepts by which historical hermeneutics is made possible, etc. The reader's attention is arrested by Croce's strictures against one of his most orthodox (and latterly apostatic) former disciples, Guido de Ruggiero (pp. 37–38); by the denial of Hegel's panlogism (p. 44); by the parallel, wholly in disfavor of Gentile, between Spaventa's and Gentile's neohegelianisms (p. 45); by Croce's *moue de dégoût* at Hegel's theologism; by his aversion to, and scornful condemnation of, Kierkegaard, and the disallowance of any attempt at comparing Kierkegaard with Hegel; by Croce's "shivers" at Jaspers' pronouncements on history; and, *dulcis in fundo,* by his consternation at Bergson's frank confession of unsullied innocence: "Je vous avoue que je n'ai jamais lu Hegel. Il faudra bien le lire."

ELIO GIANTURCO

WASHINGTON, D. C.

Libertà ideale e libertà storica. CONSALVO CECI. Bari: Laterza. 1950. 195 pp.

Deprivation of the sense of material, external sight seems to have increased the intensity of the spiritual, internal vision of Consalvo Ceci (1894–1948), a heretofore little-known personality, author of the present volume on *Ideal freedom and historical freedom,* which Croce recommends as "perhaps the best exposé of the philosophical concept of freedom, considered in all its aspects and relationships and in its vital links with past and current history."

Ceci's treatment consists of a *pars construens* and a *pars destruens.* The latter is a polemic against Guido Calogero's doctrine of freedom, based on voluntaristic premises, contained in his *La Scuola dell'Uomo* (Firenze, Sansoni, 1939), and developed in his *Etica, giuridica, politica* (Torino, Einaudi, 1946). Ceci's objections do not undertake to link Calogero's theory of freedom with the latter's epistemology or metaphysics. A formidable caveat is entered against prospective refuters of Ceci's criticism (pp. 69–70). In the *pars construens,* the fine pages on activism and war (pp. 154 ff.); on freedom and civilization (p. 95: the growth of legislation and organization is not, *per se,* unfavorable to the increase of free activities); on freedom and law (p. 103: every human action implies a legal element); and the passage setting forth the author's noble human creed (pp. 108–185) must be singled out for special mention.

ELIO GIANTURCO

WASHINGTON, D. C.

The Ego and the Self. PERCIVAL M. SYMONDS. New York: Appleton-Century-Crofts, Inc. 1951. ix + 229 pp. $2.50.

A good, readable text on an important psychological topic—the person as an object of scientific observation (the ego) and as an object of his own awareness (the self). It is eclectic but predominantly Freudian in orientation, with a generally constructive approach, and contains good summary accounts of most of the recent experimental work relating to various phases of the subject. The book has a good index and a bibliography which includes brief, critical notes on each reference. Its principal weakness is the author's uncritical acceptance of the vitalistic, fluid-theory concepts of Freud, without due consideration of recent criticisms of these by Clara Thompson, Karen Horney, *et al.* Another weakness is his failure to appreciate the basic theory of the gestalt psychologists—for instance, he shows little awareness of what was involved in their experimental work on perception and insight.

Interesting philosophically is Professor Symond's *saying* that the ego is process but *treating* it as an undetermined causal (spirit), exercising active directive functions without itself being causally determined in the exercise of these functions. It is not easy to combine the notion that the person is a natural part of things (essential to a *scientific* treatment of the person) with the notion that he is an active agent exerting a significant influence on the course of events (essential to a *moral* treatment of the person). If the person is to be seen as a natural part of things, his origin and functioning must be seen as explainable in terms of either linear or field-type causality. This combination is especially difficult within the framework of Freudian libido concepts since these rest on *unknowables*—basic energy drives conceived as weightless fluids, the ego as an undetermined, autonomous causal agent devoid of energy, the super-ego as a blind causal agent determined by external factors, etc. Further, it is questionable whether the Freudian concepts can provide an adequate basis for seeing the self as a significant factor in the course of events. That these concepts are not devoid of heuristic value must be granted; nevertheless, it would seem that we must go beyond them. In his eclecticism Professor Symonds does go a little beyond them, and his own view of human nature is generally more optimistic than was Freud's, but further scientific work, possibly along gestalt lines, needs to be done.

MORRIS R. SHORT

WASHINGTON, D. C.

NEW BOOKS AND CURRENT JOURNALS

Wendell C. Bennett: Area Studies in American Universities. New York: Social Science Research Council. 1951. x + 82 pp. $1.00.

Richard Foster Jones (and Other Writing in his Honor): The Seventeenth Century. Studies in the History of English Thought and Literature from Bacon to Pope. viii + 392 pp. $7.00. Stanford, California: Stanford University Press. 1951.

N. O. Lossky: History of Russian Philosophy. New York: International Universities Press, Inc. 1951. 416 pp. $10.00.

J. A. Passmore: Ralph Cudworth. An Interpretation. Cambridge; At the University Press. 1950. ix + 118 pp.

H. J. Paton: Kant's Metaphysic of Experience. A Commentary on the First Half of the Kritik der Reinen Vernunft. Two Volumes. (The Muirhead Library of Philosophy.) London: George

Allen & Unwin, Ltd. New York: The Macmillan Company. First published in 1936. Second Impression, 1951. 585 pp.; 510 pp.

A. S. L. Farquharson: Marcus Aurelius. His Life and His World. New York: William Salloch. vi + 154 pp. $2.00.

Rainer Maria Rilke: Letters to Benvenuta. New York: Philosophical Library. 1951. 87 pp. $2.75.

Ranjee Shahani: The Indian Way. New York: Philosophical Library. $3.75. 175 pp.

Robert Lawson Slater: Paradox and Nirvana. A Study of Religious Ultimates with Special Reference to Burmese Buddhism. Chicago, Illinois: University of Chicago Press. xi + 145 pp. $4.00.

Claude W. Thompson: Humanism in Action. New York: Pitman Publishing Corporation. 1951. 275 pp. $3.75.

PHILOSOPHY OF SCIENCE. Vol. 18, No. 3. Professor Margenau and the Problem of Physical Reality: *W. H. Werkmeister.* Biology and Unitary Principle: *R. S. Lillie.* Relativity and Representativeness: *K. R. Hammond.* Note on Hammond's Analogy between "Relativity and Representativeness": *Egon Brunswik.* Contemporary Philosophy and Philosophy of Science: *J. S. Adams.* Form and Content in Empirical Science: *H. L. Searles.* Mechanism, Organism, and Society: Some Models in Natural and Social Science: *K. W. Deutsch.* The Experiment and its Role in the Theory of Knowledge: *Silvo Fiala.* Coöperative Research in the Science of Religion: *G. Y. Rusk.* Discussion—The Psycho-Linguistic Movement in Holland: *D. Vuysje.*

PHILOSOPHICAL REVIEW. Volume LX, Number 3. A Finitistic Approach to Philosophical Theses: *Morton White.* Philosophers and Ordinary Language: *R. M. Chisholm.* Philosophy for Philosophers: *Norman Malcolm.* The Empirical Basis of Anselm's Arguments: *H. G. Wolz.* Discussion—Carnap's Theory of Probability: *Georg Henrik von Wright.* Formal Logic and Dewey's *Logic: M. G. Singer.* Was Spinoza a Nominalist? *J. K. Feibleman.*

AUSTRALASIAN JOURNAL OF PHILOSOPHY. Vol. XXIX, No. 1. Nature and Convention in the Democratic State: *A. Boyce Gibson.* Psychology and Poetry: *A. R. Crane.* Heinrich Hertz and the Concept of Force: *J. J. C. Smart.*

GIORNALE CRITICO DELLA FILOSOFIA ITALIANA. Anno XXX, Volume V. Il mito dell'assoluto e la problematicità dell'esistenza: *Vittorio Sainati.* Il problema soggetto-oggetto e le aporie della sintesi kantiana: *Carmelo Lacorte.*